LETTERS TO THE EDITOR.

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The University of London.

I HAVE been away from home, and have only now seen Mr. Thiselton-Dyer's letter of August 23.

My previous letters were, I thought, quite clear; but as he asks me to do so, I write to explain that my two statements which he quotes, viz.: (1) "I am not asking that any privilege which they do not at present possess should be conferred upon my constituents, but only supporting what is now their legal right. . . . This right I know they highly value"; and (2) "It is the law at present," had reference to the present right of veto possessed by Convocation.

As regards the vote being taken as at a senatorial election, so far from stating that this was at present the law, the very terms of my letter implied that it was a change.

Whether it would be "radical" or "revolutionary" is, of course, a matter of opinion, but I certainly did not make the suggestion with the object attributed to me; nor do I share my friend's opinion that the graduates would take a course which, to quote his words, "would destroy the prospects of Academic study in London." JOHN LUBBOCK.

High Elms, October 8.

Sir Robert Ball, and "The Cause of an Ice Age."

MR. JAMES GEIKIE has recently brought out another edition of his "Ice Age," a well-known and influential work. In this book he quotes freely from Sir Robert Ball's "The Cause of an Ice Age," which appeared in 1891, and which was remarkable as the first work written by a professed astronomer in which an astronomical explanation of an Ice age was put forward and defended. As the influence of these books upon popular opinion, and even perhaps upon some scientific men, may prove very misleading and mischievous, perhaps you will allow me a little space in which to discuss Sir Robert Ball's work.

The book was preceded by much advertisement, in which we were told not only that it contained an entirely new view of the subject, but that an astronomical basis of the Ice theory was at last securely established.

When the book itself was published, it appeared also that the new matter in it consisted of "a law, hitherto unsuspected, regulating the distribution of heat between summer and winter in either hemisphere." Thus on page 113 the author says: "*I discovered* the law of distribution of sun heat on a hemisphere between the two seasons into which the year is divided by the equinoxes." Again he says: "*I enumerated and proved* that law of the distribution of sun heat between the two seasons, which I have already referred to as the cardinal features of this little book" (*op. cit.* 113.) Again, in the appendix he says: "The following is the calculation often referred to in this book, and in which *for the first time*, so far as I know, the astronomical facts relating to Ice ages have been correctly given." Lastly, he says: "If it should prove that the facts which these numbers imply have not been given by any previous writer, *then their announcement is the novelty in this book, the one central feature by which it is to be judged.*" Sir Robert Ball afterwards speaks slightingly of Herschel and Croll for having ignored this law.

It was very soon pointed out in a review of his book that this particular law which Sir R. Ball claimed to have discovered had been already enunciated and published by Wiener.

This fact might easily have escaped any one else but a writer who was himself a mathematician writing expressly on this very point, which was the justification of his book. Let that pass, however.

It seems to some of us that when the Astronomer Royal for Ireland had had this pointed out to him, he ought at once to have written to the scientific papers correcting his mistake, and doing justice to the real discoverer of the law, and that the book itself should not have been issued again without this correction appearing in it, for the publication of the supposed discovery was the *raison d'être* of the book.

Nothing of the kind has happened, however, and the only correction that I know of the mistake by its author is in an *astronomical* book published in 1893, entitled "The Story of the Sun," in which no reference whatever is made to the claims set up in 1891, but the law in question is simply referred to as "Wiener's law," as if everybody in the world must know that Wiener and not Ball had discovered it. Meanwhile, "The Cause of an Ice Age" is not cancelled or withdrawn or corrected, but is being continually issued with all its exploded claims.

What I have just written refers merely to a claim to have discovered a law which was discovered by some one else, and to the amenities which generally regulate our conduct when we are shown in such a case to have done another man an injustice. But this is a very small matter. A much more important matter remains.

The law which Sir R. Ball claimed to have discovered is an indisputable one. No one doubts it, or could doubt it. What most people who have examined the problem say, however, is not that the law is not a perfectly good one, but that it has nothing whatever to do with the question of an Ice age. The law in question is briefly, that the quantity of heat received by either hemisphere of the earth in summer is to that it receives in winter in the ratio of 63 to 37. This is an invariable ratio, true at all times, and true under all conditions of eccentricity of the orbit. It never varies. It was the same millions of years ago, so far as we know, as it is now, and so it will remain. It is therefore a *constant* factor in the problem, and being a *constant* factor it cannot be the cause of *variability* of climate. If, as we are told in the book over and over again, this particular proportion is the cause of an Ice age, we must be living in an Ice age now, and we must always have been in an Ice age. Therefore the law in question was not only not new, but it is an absolutely irrelevant law so far as the problem at issue is concerned. Whether the particular numerical ratio was present to the minds of Herschel and of Croll when they wrote on the problem, is quite immaterial; and being so, the whole *raison d'être* of Sir R. Ball's book is gone, and so far as we know there is not a single material factor of the problem discussed by Sir Robert Ball which was not present to Croll when he wrote "Climate and Time" and his other works.

Lastly. Sir Robert Ball, following in the wake of Croll, has subjected the various facts and conditions, both astronomical and meteorological, which in his view induced an Ice age to analysis, and has reached certain conclusions which he has emphasised in his later work, "The Story of the Sun." This analysis has been criticised and examined by more than one person, but with especial closeness of reasoning and conclusiveness by one of Sir R. Ball's own pupils, a distinguished Fellow of Trinity College, Dublin, Mr. Culverwell. His criticisms have appeared in NATURE and in the *Geological Magazine.*

In the view of those who have read these criticisms, they are simply crushing. No more complete and acute dissection and destruction of a scientific argument has appeared for many years.

This criticism was originally read at the British Association, in the presence of Sir R. Ball himself, who made no attempt whatever to answer it, but (mistaking his audience) merely gave vent to some jocular remarks. The Lowndean Professor at Cambridge cannot turn the flank of serious criticism by ill-timed jokes. Since then he has not, so far as I know, answered his critics in any way, or tried to justify his riddled arguments, and the books in which they are contained are being sold, and their conclusions are being quoted as if they were sound instead of being absolutely untenable.

If Sir R. Ball were an ordinary person, a free lance in literature and science, he might say anything and publish anything with impunity, and might refuse to answer criticism from any quarter; but he was once Astronomer Royal for Ireland. He now fills the chair at Cambridge once occupied by Adams. He cannot write without in some way committing that chair and that University by his opinions; and his principal critic is not an obscure scribbler, but a mathematician as accomplished as himself. Is it right or decent that, under these circumstances, he should continue to publish, with his name on the title-pages, works such as those I have described? Ought he not either to at once confess his mistakes, to answer his critics; or if he cannot do this, to withdraw books which have done some harm to thoughtless people, which have brought no credit to the chair he fills, nor to the University of which he is a Professor; and which have given rise to a good deal of angry comment among those who do not understand a man of science, of real distinction, remaining, for a day longer than he can help, the foster-father of what has been shown to be wrong either in fact or in argument?

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I do not think Sir John Lubbock can know the facts of the case, or he would not permit his name to appear as the god-parent of a book thus flyblown; nor should its publishers continue to issue it, and this not because the book contains mistakes—all books do that—but because its mistakes have been pointed out, and because its author is a great deal more than Sir Robert Ball, and cannot therefore escape the penalty of such a position.

The Athenæum Club, HENRY H. HOWORTH.
October 4.

MacCullagh's Theory of Double Refraction.

AN attempt has recently been made by Mr. Larmor to resuscitate MacCullagh's dynamical theory of double refraction (Brit. Assoc. Rep., 1893; *Phil. Trans.*, 1894, A, part ii.), but on examination this theory appears to me to infringe one of the fundamental principles of dynamics, viz. the principle of angular momentum.

Whatever the constitution of the medium may be, the forces which act upon any element consist of two distinct classes: (1) forces due to the action of contiguous parts of the medium; (2) forces arising from causes external to the element. The forces comprised in the first class are usually termed stresses; they act upon the surface of the element, and are completely specified by the nine quantities X_x, X_y, &c. The forces comprised in the second class act upon each element of mass, and arise from attraction or repulsion due to external causes or to the action of the medium upon itself. These forces, from whatever cause they may arise, are capable of being compounded into a single force along a line through the centre of inertia of the element, and a couple about some axis through this point. In ordinary gravitating matter the couple vanishes.

The equations of motion of the element in terms of the stresses and the *force constituent* of external action are the analytical expressions for the principle of linear momentum; but this principle is not sufficient to determine the motion of the medium—it is further necessary to satisfy the principle of angular momentum, and any theory which violates the latter principle is dynamically unsound. Now the principle of angular momentum requires that three relations of the form $X_y = Y_x$ should exist between the six shearing stresses, thereby reducing their number from six to three, except in the following two special cases. The first case occurs when the medium, previously to being disturbed by the passage of a wave of light, is *not at rest*, but possesses an independent angular momentum; that is to say, the medium is what has been termed a *gyrostatic* one. The second case occurs when the resultant of the external forces which act upon the element consists of a *couple* as well as a force. In the first case the kinetic energy of the disturbed motion of an element will not be proportional to the square of its velocity of translation, but will contain a term depending on the gyrostatic momentum; whilst in the second case the potential energy must necessarily contain a term due to external action.

Mr. Larmor assumes that the kinetic energy of an element is proportional to the square of its velocity of translation, so that the medium he considers is not a gyrostatic one; whilst the potential energy is supposed to be a quadratic function of the rotations, and he obtains his equations of motion by means of the principle of least action. Now, as we have pointed out, the potential energy of an element *may* consist of two distinct parts, viz. one due to deformation, and the other due to the action of external causes; and it is quite legitimate to assume *by way of trial* that the former part contains rotational terms. But it is well known that a quadratic expression which contains rotational terms will not satisfy the conjugate relation between the six shearing stresses, and consequently the principle of angular momentum will be violated, unless every element of the medium is under the influence of some system of forces, of the kind belonging to the second class, the couple constituent of whose resultant *does not vanish*. The potential energy ought therefore to be of the form $W + V$, where W is the portion due to deformation, whilst V is the portion due to external causes which supplies the couple which is necessary in order to prevent the principle of angular momentum being violated; and unless Mr. Larmor is able to surmount this difficulty, I am at a loss to understand how his paper is an improvement upon theories which are at any rate *dynamically sound*, whatever other imperfections they may possess. The question is one which cannot be disposed of by pages of vague and obscure generalities, but requires a detailed and careful mathematical investigation for its elucidation.

A. B. BASSET.

Holyport, Berks, October 3.

The Southern Carboniferous Flora.

So far as I am aware, Dr. Kurtz's paper on the newly discovered Carboniferous Flora in Argentina had not been noticed in print in this country until the appearance of the number of NATURE for September 26, which contained a note (p. 523) giving a brief abstract from the translation published in the Records of the Geological Survey of India. The circumstance that the original paper, which appeared nearly a year ago, was in Spanish, may have caused its being overlooked.

The subject of the ancient Southern floras is naturally unfamiliar to most European geologists, and I hope I may be allowed to point out why the present discovery is important. It completes a mass of evidence gradually accumulated. It is, of course, well known that several successive floras of Upper Palæozoic and Lower and Middle Mesozoic Age have been found associated with beds mainly of freshwater origin, some of which combine valuable coal seams, in India, Australia, and South Africa. The most ancient of these beds in Australia and South Africa contain certain plants, amongst them a *Lepidodendron*, allied to the ordinary Carboniferous flora of Europe and North America. From the upper beds in all the three regions named, Ferns, Cycads, and a few other plants have been obtained that are related to the Rhætic and Jurassic types found in European rocks. Between the upper and lower plant-bearing strata in South Africa and Australia, and beneath the upper series in India, are found beds, with coal seams in places, containing by far the most remarkable flora of the whole, the Glossopteris-flora, as it has been called. The particular interest attaching to this flora is mainly due to two circumstances. (1) It is clearly Upper Palæozoic, for in Australia the coal measures containing it are interstratified with marine beds abounding in carboniferous fossils, and yet it differs radically from any known European or North American flora of that age. (2) The basal beds, in India, Australia, and South Africa, are boulder beds, resembling the Pleistocene glacial boulder clay more than they do any other formation.

Now in Argentina the occurrence of the Southern Jurassic or Rhætic flora has been known for some years, and Prof. Derby has called attention to the presence in Southern Brazil of a great boulder bed, that very probably corresponds in character and geological position to the Talchir beds of India and the Dwyka beds of South Africa. More recently traces of the ancient Lepidodendron flora have been discovered in Argentina, and some additions to that flora are described in Dr. Kurtz's paper. But the important announcement in this paper is the discovery in Argentina of three Indian lower Gondwána plants, *Neuropteridium validum*, *Gangamopteris cyclopteroides*, and *Nœggerathiopsis hislopi*, all three associated in India with the Karharbári coal-seams near the base of the Lower Gondwána. Two of the species are also found or represented by closely allied forms in Australia and South Africa. In Argentina, as in India, Australia, and South Africa, there is a remarkable absence in this particular flora of forms characteristic of the Upper Palæozoic of Europe, no representative of Lepidodendron or Sigillaria occurs, and the Ferns, Cycads, and Equisetaceæ that constitute the flora are related to European Mesozoic types.

It is difficult to understand how two floras differing from each other far more widely than do any two continental floras living on the earth's surface at the present day, can have coexisted unless there was, for a long period of geological time, a great southern continent—the Gondwána-land of Suess—isolated by a wide sea, probably an ocean, from the land that occupied in Carboniferous and Permian days so wide an area in the northern hemisphere. The importance of the new discovery is the immense extension that it gives to Gondwána-land, and the proof it affords that the region with its flora extended to the western hemisphere, and included a part, at all events, of South America. This appears to indicate that a considerable area now occupied by ocean in the southern hemisphere was land in the Carboniferous period. Further research is needed to show whether the various tracts of Gondwána-land were connected by a South Polar land area.

W. T. BLANFORD.

October 4.

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